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TRANSLATIONS.

Anatomical and Physiological Observations made upon Decapitated Subjects.

BY M. C. ROBIN.

From Robin's Journal de Physiol, etc.

I—The Condition of the Cervical and Cephalic Veins.

When the cervical and cephalic veins of decapitated subjects are examined several facts are observed which possess a certain degree of interest in a physiological point of view. Upon the cut surface there may be seen veins clearly cut whose walls are there more or less depressed. The corresponding arteries have their cut extremities retracted to the depth of about a centimetre into the thickness of the tissues.

But what deserves especially to attract the attention of observers is this: that the internal jugular vein incompletely retracted upon itself has its cavity full of air. This re-

placed the blood at the moment of the abrupt pouring out of the liquid; the air remains there, as is revealed by dissection, by reason of the habitual state of tension of the cervical aponeurosis due to its insertion, and which maintains the veins of the neck in a state of distension above the os hyoides. This has been noticed especially by M. Malgaigne (*Anatomic Chirurgicale*, Paris, 8vo, 1859, 2d edit: t. II., p. 128), in discussing the anatomical observations upon this subject made by M. Bérard (1830), De Grusse (Thése 1849), Richet, etc. Dissection shows, sometimes, also, a little air, in bubbles, mingled with the blood in the trunks of the principal branches which empty into the internal jugular, such as the facial and temporo maxillary veins. The same also sometimes occurs in the veins which surround the transverse apophyses of the first cervical vertebræ, and communicate with the *internal jugular* by means of the inosculating branch which returns from the last to the superior part of the *posterior jugular*.

The most constant fact actually to be noticed consists in the presence of air in the fossa of the jugular vein, in the lateral and other sinuses of the base of the cranium, as well as in the two longitudinal sinuses.

From thence large bubbles of air may be followed in the veins of the cerebral and cerebellar pia mater, and even into several of their subdivisions, along the fissures separating the convolutions.

In the last subject examined I discovered, one hour after death, large bubbles of air in the right sinus, as far as the vena Galien, and in the two large choroid branches. I have never seen them in the veins of the ventricular walls, nor in those which plunge into the gray and white cerebral substance, whatever their volume may have been.

Thus air takes the place of blood wherever the anatomical arrangements are such that the organs of the surface of the body cannot be depressed upon the veins in proportion as the liquid escapes from the vessels abruptly opened. In

place of blood, which, in bodies of subjects dead from disease, fills, more or less fully, the vessels, air, alone or mingled with blood, is found under the conditions of death examined here. As to the smaller intra-cranial vessels, other than those referred to, they remain (as in dead bodies generally) more full of blood relatively than those in the extra-cranial regions, the incompressibility of the cranial case, in reference to atmospheric pressure, opposing an obstacle to their retraction, and consequently to the expulsion of the blood in the large venous trunks. This fact is observed, as is well known, even in men and in animals dead from hæmorrhage in consequence of arterial wounds of the limbs.

In this last relation especially, the intra-cranial vessels, other than the sinuses, are not, in decapitated subjects, removed from the influence of atmospheric pressure, as in individuals dead from disease. There is found, indeed, in the first, air, in the place which was occupied by the cephalo-rachidian liquid around the spinal marrow, in the posterior sub-arachnoidean space, and more or less in the spaces at the base of the cranium.

Finally, it must be added, to conclude what refers to the condition of the veins, that the trunk of the vertebral vein adherent to surrounding parts, is slightly retracted upon itself and contains air.

I recall very clearly that my attention was attracted to the presence of air in the cranial sinuses and in the veins of the pia-mater, in 1844 and 1845 by MM. P. Bérard and Denonvilliers. Since then I have verified these facts every time I have had occasion to dissect decapitated persons, and three times in particular, during the last two years, I have vainly sought their indications in treatises of anatomy and physiology. It has been equally impossible for me to discover where Bérard (*Eléments de Physiologie*, Paris, 1858, 2d Edit., t. II, p. 346) has drawn the elements of the following phrase, the only one alluding to the subject under dis-

cussion which has come within my knowledge: "Examine," says Bérard, "the head of a decapitated subject, as Beclard, Abercrombie and Professor Bérard have done, or even yet examine the cranial cavity of a person dead of hæmorrhage and you will find there, always, a large quantity of blood."

It is perfectly true that the intra-cranial venous vessels of subjects decapitated, are not absolutely drained of blood. There remains some blood in the smaller veins of the pia-mater, equally in those penetrating the gray and the white substance. But this quantity is sensibly less than in the majority of subjects, dead of different diseases and more or less emaciated, destined for dissection. Moreover, as we have just seen, the cranial sinuses are full of air, or contain more air than blood; the same is true of several of the veins which empty into them, and many of the branches of these last contain bubbles of air, more or less numerous, mingled with blood, filling them for a certain length, and separated from each other by little columns of blood.

II—The Conditions of the Cervical and Cephalic Arteries—Physiological Consequences of this Condition.

In connection with MM. Legros and Goujon we have observed, in two decapitated subjects, another series of facts more unexpected than the preceding, concerning the presence of air in the vessels. We saw air mingled with blood in the arteries of the head, in the cerebral and cerebellar arteries as far as the arterioles of the pia-mater. M. Legros particularly, investigated these facts with the greatest care in reference to the experiments which he had made in connection with M. Omimus, upon arterial circulation (See *Journal of Anat. and Physiol.*, 1868, p. 371) upon the head of a decapitated person whom I dissected two hours after execution, in September last, with especial reference to the study of the state of the vessels. I determined that the internal and external carotid arteries, incompletely retracted upon themselves, contained frothy blood in which the vol-

ume of air surpassed, considerably, that of the blood. All the branches of the arterial hexagon (circle of Willis) of the base of the cranium, very sensibly retracted upon themselves, contained blood mingled with a certain number of air bubbles. The origin of the ophthalmic artery was in the same condition. The arteries originating from the anterior portion and from behind the hexagon were considerably retracted upon themselves without being completely emptied of blood, and their trunks had the appearance of little brownish resistant cords about a millimetre and a half in thickness.

Upon the two other decapitated subjects which I had examined previously with MM. Legros and Goujon, the arteries springing from the hexagon were a little less retracted, and contained bubbles of air, at least equal in quantity to that of the blood with which it was mingled.

The trunks of the principal branches of the external carotid contained blood mingled with bubbles of air. This fact was very striking in the maxillary, and especially in the middle meningeal artery, whose branches contained bubbles of air almost up to the middle of the sides of the cranial arch.

Section of the vertebral arteries made nearly at the level of the transverse apophysis of the fourth cervical vertebræ showed at each side an open orifice alongside of that of the vein: the artery incompletely retracted upon itself in consequence of its adhesion to surrounding organs, contained more air than blood nearly up to the basilar trunk. The latter, much more retracted upon itself than the preceding arteries, contained, like the branches of the hexagon, little columns of blood separated by bubbles of air. The same was true of the cerebellar arteries, but only at their origin.

It is perfectly easy to comprehend under what point of view these facts are worthy of interest.

All physiologists know the remarkable experiments pro-

posed by Legallois, and so well executed for the first time by Brown-Sequard (*Journal de la Physiologie*, Paris, 1858, 8vo, p. 119), which is thus described:

"I decapitated a dog, *taking care to make the section below the point where the vertebral arteries penetrate the osseous canal.* Eight minutes after, pinching of the skin being without effect, I applied a galvanic current of very considerable intensity, to the exposed medulla oblongata, avoiding carefully the passage of the current through the adjacent part. He manifested no movement. The conductors applied to the protuberence could produce no effect. Ten minutes after the cessation of the respiratory movements of the nostrils, of the lips and of the lower jaw, I fitted to the four arterial trunks of the head canulæ which were connected by tubes of caoutchouc with a brass cylinder, by means of which I injected blood charged with oxygen by the aid of a syringe. In two or three minutes, after some slight disorderly movements, I detected movements of the eyes and of muscles of the face which seemed to be directed by the will. I prolonged the experiment a quarter of an hour, and during all that period these movements, apparently voluntary, continued to take place. After having discontinued the injection these movements ceased, and were soon replaced by convulsions of the eyes and of the face, by respiratory movements of the nostrils, of the lips and of the jaws, and, finally, by the tremors of the death-struggle. The pupil dilated and contracted as in ordinary death.

This experiment, adds Brown-Sequard, demonstrates positively the possibility of the return of the vital properties and of the functions of the encephalon under the influence of blood charged with oxygen."

There can be no doubt that this experiment, made upon man under conditions analogous to those in which Brown-Sequard was placed, would give results of the same order,

as to the phenomena of central innervation, and of motor excitation in particular.*

It is very certain that movements of the lids and lips may be determined under these conditions, like the return of the nervous tissue to reflex actions, if not to voluntary movements, by tickling or pinching the skin at this or that point of the face; movements obtained in an analogous manner to that which Brown-Sequard (Comptes rendu de l'Academie des Sciences, 1851, t. XXXII, p. 856), has perceived whilst making experiments of this kind upon the posterior portion of the bodies of rabbits.

But it will be seen after what has been already said of the presence of air in the vessels of the neck and of the cranial cavity, that contrary to what some physiologists have thought from the indications of M. Velpeau (Leçons sur la physiologie du System Nervoux, Paris, 1866, 8vo., p. 460,) this experiment could not be made upon the head of a decapitated person. It could not succeed, or at least furnish conclusive results, as upon the body of a man killed by balls striking below the neck. Under such circumstances only, which it would not be impossible to meet, section of

*It may be added that this experiment, and others likewise, which Brown-Sequard has described in this same work, demonstrate the functional anatomy of the central and peripheral nervous cells and tubes, in the same manner as that of other species of anatomical elements whose properties are manifested while the characteristic muscular condition of the state of organization persist, and the relations of the element with the blood permit its incessant molecular renovation. It is sufficient that these relations cease, in order that the elements should lose their properties, and these which would not return, if this state were maintained, would re-appear just in so far as these relations were re-established, before the chemical decomposition of this or that principle in the substance of the element may have taken place. Then, whilst it is yet too slight to modify the form or the structure of the elements, this decomposition is, nevertheless, sufficient already to destroy the existence of the particular molecular state which is essential to the state of organization: in order that consequently the conditions of this continuous renovation should no longer exist: aside from the accomplishment of which no other property of the organic series can manifest itself.

the neck might be performed, *below the level at which the vertebral arteries penetrate their canal*, conditions of which Brown-Sequard has not misunderstood the importance.

It is indispensable indeed to divide the arteries at one point, and even to effect it in such a manner that the retraction of their walls may be accomplished in proportion as the blood pours out, so that this last may not be partially filled with air as occurs in the case of guillotined subjects, through the vertebrals.

It is likewise so with the carotids which, instantaneously and simultaneously cut transversely and entirely, lose immediately their blood under conditions which are no where met with outside of this mode of capital execution.

Now for the vertebrals this retraction is not possible, nor at least is not accomplished except after a prolonged traction exercised upon the surrounding layers of tissue throughout their whole extent, in the foramina at the base of the transverse cervical apophyses. Cut at one point of this place, they invariably permit the air to enter in place of the blood which escapes, and by reason of the incompressibility of the cranial vault, this air accumulates thence in the intra-cerebral arteries which empty themselves more or less in different individuals.

Although the conditions are not the same in this relation for the carotids, it is however certain that, in decapitated subjects they are not retracted upon themselves up to the point of complete obliteration, and that they contain air which extends into these or those branches as we have already seen.

Hence the impossibility of making into the head of a decapitated person, a few minutes after death, an injection capable of giving results such as those that M. Brown-Sequard has obtained under the conditions which he has noted.

It is well known indeed that in every injection made into the arteries or veins, one of the essential precautions to be

taken is to prevent the entrance of air before the column of liquid is introduced. Otherwise, as occurs both in the living and the dead, the presence of bubbles or of a column of air is seen in the little vessels or in the capillaries opposing an obstacle to the progression of the liquid, and soon the continuation of the pressure induces ruptures of the tubes with foci of extravasation, or with infiltration of the fluid into the elements of the surrounding tissues. Hence artificial apoplectic extravasations going in this case counter to the intention of the experiment; for the result would not be certainly comparable to those which experiments made upon dogs have furnished, and from thence one would be led to conclude dissimilarities which do not exist between the fundamental acts of innervation. The results might even be negative, without permitting the deduction of any logical conclusion therefrom.

Thus the circumstances in which the vessels are cut in decapitated subjects are such that the air takes the place of the blood which jets out; and that, too, not only in the veins but likewise in the arteries into which blood ought to be forced in order to replace the nervous elements in conditions of internal constitution such that their extinct properties may reappear.

The conditions which render the experiment valuable and conclusive fail then here, and those which exist constitute in this particular case difficulties impossible to surmount in order to attain the desired result. This is moreover but one more illustration among those which show how often, in the conception and in the execution of every experiment, it is necessary to take into consideration the conditions of accomplishment of the phenomenon which one endeavors to modify in order to analyze it more clearly, and how, in this respect, no intellectual superiority can be substituted for the observance of these conditions of organic, chemical and physical order.

It may be considered, however, as certain that the phe-

nomena of central innervation placed in evidence by the movements observed in the dog during the experiment of M. Brown-Sequard, and those which could be established in man by placing him in similar conditions could not be regarded as associated and co-ordinated into thoughts becoming motors of the voluntary execution of this or that movement.

For those who have studied, as have physiologists and physicians, the conditions, as well intrinsic as extrinsic, necessary to the regularity of intellectual acts, the solitary fact of the absence of the cephalo-rachidian liquid in the head separated from the trunk, suffices independently of questions of pressure and of the regularity of the course of the blood injected, to lead to the belief that the acts of cerebral innervation which are manifested then, are hardly comparable but to those of which men do not preserve the recollection, and which take place, for example, at the beginning of the cessation of a syncope, as also in delirium, during an access of fever, etc.

[CONTINUED IN NEXT NUMBER.]

SOCIETY REPORTS.

Chicago Medical Society.

FRIDAY EVENING, May 28, 1869.

Regular weekly meeting of Chicago Medical Society, President Bogue in the chair. Proposals for new members being in order, Dr. A. I. Brackett was proposed by Drs. Schmidt and Paoli, and referred to board of censors. Discussion for the evening being now in order, viz., "What is the best treatment of Leucorrhœa," Dr. Trediche stated the

disease might arise from a cancerous or syphilitic diathesis; the discharge might proceed from the cavity of the fundus body or neck of the uterus—from the vagina or pudenda; the disease might be functional or inflammatory; acute or chronic. Treatment: where there is plethora, saline purgatives, or abstraction of blood. Mild vaginal enæmas are best adapted in the acute form. In the chronic variety, where the discharge is excessive from the uterus, both constitutional and local measures are indicated, viz., ferruginous tonics and supporting diet, opium, conium, and hyoscyamus to allay pain. Locally, argent nitras in solution or solid stick to the part affected; decoctions oak bark and blisters to the sacrum, where the cause was displacement or procedentia uteri, to reposit the organ. Dr. Merriman stated leucorrhœa was catarrhal in its nature, and requires constitutional treatment; that urethal, etc., discharge proceeds from the cavity of the uterus or vagina; that it should be treated as a nervous disease: iron, which is an astringent, also strychnine, nux vomica and arsenic, which have control over the peripheral, nervous filaments and arterioles; that the treatment should be persistent. As the disease becomes local the most soothing medicines are indicated, viz., the warm sitz baths, vaginal anodynes and astringent enæmas, and insufflation of the powder of bismuth. Dr. Gray believes it a local disease, and has treated it as such with success. Dr. Foster stated that in mechanical displacement of the organ, giving rise to leucorrhœa, to reposit the organ was the only remedy, that where the disease approaches gonorrhœa local anodynes and astringents cure. Dr. Mitchell stated where the disease was incident to those local affections of the skin, arsenic was a good remedy. Dr. Paoli stated leucorrhœa was not a disease but a symptom; that we seldom saw it in the acute form; that the chronic variety was most persistent. The patient is generally anemic, and prone to pulmonary affections; that the symptoms follow frequent abortions, metritis and mechan-

ical displacements, procidentia uteri, etc. Dr. Fitch stated the disease may be either local or constitutional and should be treated accordingly; that there was a want of tonicity in the system, and a watery condition of the blood. The constitutional treatment should consist of iron, general tonics and liberal diet. In cervical discharges prefers the local use of solid argentinitras within the cervix, but never injections into the uterine cavity; where discharges were offensive had used vaginal enæmas of solution permanganate of potassa. Society adjourned.

HIRAM WANZER, *Secretary.*

North Iowa Medical Society.

The North Iowa Medical Society held its eleventh annual session at Monona, on Wednesday, the 3d of June. Meeting called in the office of Dr. Scott. Roll called—Minutes of last meeting read and approved.

Drs. Lewis L. Hazeltine, of McGregor, Herbert S. Hill, of Elkader and Dixon Alexander, of Fayette, were duly elected members of the Society.

Dr. Scott presented a little girl with a severe gun-shot wound of the hand, which he dressed, assisted by Green, Lewis and Hazeltine; the two middle fingers amputated through the metacarpal bones

Dr. Andros introduced a patient with chronic periostitis of the bones of the wrist. Most of the members gave their diagnosis and treatment in writing.

The Society then adjourned to meet at one o'clock, P. M., in the Masonic Hall. Meeting called at the appointed hour.

Dr. D. Mason, of Prairie du Chien, Wis., was elected Honorary Member.

A complaint was presented against Dr. J. Hicks, of Monona, which was referred to the Board of Censors, to draft resolutions to present to the meeting. Dr. Hicks was notified by the Secretary to appear and answer to the charges preferred against him. He would not appear.

The Society then passed the following resolutions:

WHEREAS, Dr. J. Hicks, of Monona, a member of this Society has been convicted of a *gross* violation of the code of the National Medical Association, said code being adopted by this Society, therefore, be it

Resolved, That he is hereby expelled from membership from this Society.

Dr. D. W. Chase, of Elkader, reported a case of progressive locomotor ataxy. Dr. Lewis a case of encephaloid disease of the forearm in a girl thirteen years of age, the diseased radius and ulna, with some of the enceph-

aloid formation was exhibited. He also reported a case of the continued use of chloroform for eight hours. Dr. Scott one of six hours.

Dr. Green a case of pyœmia, with many large diffused abscesses which discharged, in all, over two quarts of pus; the patient recovered. Dr. Andros a case of abscess of the appendix vermiformis.

Dr. Earl a case of acute meningitis, accompanied with very unusual symptoms. Dr. N. H. Knowles exemplified Dr. Earle's case by one with similar symptoms, which resulted in death. A post mortem revealed also spinal apoplexy.

Dr. Earl one of organic disease of the brain, produced by functional disease of the uterus.

Dr. Scott read a very interesting and instructive paper upon various subjects pertaining to the medical profession.

Dr. Andros took his text from the modern bible: "Suffer not little children to come unto me," read a lengthy, true, and able discourse upon the common crime of criminal abortion; showed to what an alarming extent it is now carried on, the symptoms and treatment for the different stages; the danger attending it and the effect it had upon the patient, and upon the community at large; showed by statistics that the American race is fast running out.

Many interesting cases were related, and then proceded to election, which resulted as follows:

D. W. Chase, of Elkader, President.

W. C. Earl, of Wawkon, Vice President.

J. S. Green, Postville, Secretary.

J. T. H. Scott, Monona, Treasurer.

Censors, Andros, Alexander and Lewis.

Delegates to the American Medical Association, F. Andros and N. H. Knowles.

Delegates to tho State Medical Society, Scott, Hill and Hazeltine.

Essayists, N. H. Knowles and J. S. Green.

The President was excused from reading his address for the want of time, as many of the members were compelled to leave on the train.

Meeting adjourned to meet at the City of McGregor, on the first Wednesday of June next.

J. S. GREEN, *Secretary.*

Wines and Liquors for Medicinal Use.

Attention is directed to the advertisement of C. H. SWAIN & Co., in the advertising pages of the JOURNAL. We can confidently recommend this firm as reliable and prompt in their business relations, whilst their thorough acquaintance with *spiritual matters*, long experience and integrity entitle them to a goodly share of public patronage. If alcohol, in any of its various preparations, is to be used at all in professional practice, it is certainly of the highest importance to know exactly *what* is used, precisely as with any other medicament.

ORIGINAL COMMUNICATIONS.

EDITOR CHICAGO MEDICAL JOURNAL:

DEAR SIR—In the May number of your valuable paper a letter appears signed "Geo. J. Zeigler, M.D.," in reference to nitrous oxide.

As my alleged connection with this person in regard to the preparation of this article has caused me considerable chagrin, you must allow a professional brother a little space in your paper, to relieve himself from the odium which has been fastened upon him.

About eighteen years ago my acquaintance began with Mr. Zeigler, who was then employed in the hair-dressing establishment of Mr. Pollard, in this city, and I lost sight of him for some years, when my acquaintance was renewed with him, he being, at that time, pulling teeth and shaving, at Tenth and Arch streets.

Mr. S. S. White solicited me to assist Zeigler in procuring a *patent* upon nitrous oxide, and, notwithstanding my earnest protestations, did actually proceed to Washington, apply for a patent, procure machinery, etc., at an expense of over $1,000, and in order to achieve success (at the command of Zeigler) began experimenting upon dogs, having, forty dogs at one time in the back room of his building at Arch and Sixth streets. The place was called a "Dog Hospital." It was a cruel way to treat the unfortunate canines. They were all suspended by their tails, and he (Zeigler) injected the gas *per anum*.

To those who know me intimately, I need not say that I would not do any thing so unprofessional as to attempt to secure a *patent* upon ideas as old as Sir Humphrey Davy.

I deny, most emphatically; I owe it to my professional char-

acter to do so, that I gave any counsel to either Zeigler or White in this business! On two or three occasions White induced me to re-write and re-spell Zeigler's articles. I did not receive one cent pay for it. Mr. Zeigler is, as far as I know, a worthy mechanic. "Curling Hair" is a respectable occupation when legitimately performed. But I do most heartily protest against the charge of assisting either Ziegler or White in their operations.

Yours, truly,

GEO. R. WELDING, M.D.

1372 N. Eleventh St., Philadelphia.

MISCELLANY.

The Microscope in Sterility.

[Extracts from a paper read at the Oxford meeting of the British Medical Association in August, 1868, and subsequently read before the New York Medical Society on the 7th of December, 1868. Printed first in British Medical Journal in November, and reprinted in New York Medical Journal in January, and made into a pamphlet from this reprint.—*Nashville Journal.*]

BY J. MARION SIMS, M.D.

How often have we heard it said—how often have I said it myself—"An operation is necessary in this case, because the canal of the cervix is too small to permit the easy entrance of the semen!" Now, these important questions cannot be determined with any degree of accuracy in this hap-hazard manner. For it is not always necessary to incise the cervix uteri, simply because it does not easily admit the passage of an ordinary sound; nor, on the other hand, are we justified in condemning an operation, simply because the sound can be passed easily. In other words, a very small os does not call for operation, nor does a larger one always forbid it. Do you not think, then, that a great service would be rendered, if we could deduce this question of operation, or no operation, from the broad domain of spec-

ulative opinion to the narrow path of absolute scientific certainty? There is nothing easier, for the microscope accomplishes this in the most perfect manner imaginable. It settles the question of operation, or no operation, in an instant, leaving nothing whatever to be guessed at and nothing to be desired.

Is it surprising that positive knowledge of this sort should meet with opposition among honest, earnest cultivators of medicine? Not at all. For it is ever so with any great truth. It must be first opposed, then ridiculed, after a while accepted, and then comes the time to prove that it is not new, and that the credit belongs to some one else. The truth here announced has had its day of opposition, and it must now soon take its stand as established and acknowledged.

On the subject of the microscopic examination of the utero-vaginal secretions, I have been misrepresented, maligned, and positively abused by a few, both abroad and at home; and I have been misunderstood by many who have not taken the trouble to read, to investigate, to think and to reason for themselves. And, Mr. President, under these circumstances, I cannot thank you too much for the high privilege of appearing here to explain and to defend my position by laying facts in the case before this learned Society, this great gathering of my countrymen, whose decision, I am sure, will be in accordance with truth and justice.

We may all differ honestly about abstractions, and theories, and mere opinions; but when it comes to facts and figures, there cannot long be a great difference among men of good common sense, with honesty of purpose in pursuit of truth. I have never yet been afraid of truth, however much it may conflict with prejudices, find it where I may; nor do I ever expect to see the day that I would fear to publish my convictions on any matter of professional importance, be the character of the opposition what it may; and, particularly, when I feel that these convictions are based upon facts that are immutable, and lead to results of the gravest importance to the honor of medicine, and to the advancement of knowledge. Whatever gives to any department of medicine greater exactitude, helps to raise it to the dignity of a science. And this is what I claim to have done with the microscope in this direction.

The microscope has done, and is doing, a great work in

medicine, as well as in the collateral sciences. But I know of no field in which it will be of more practical use than in the treatment of the sterile state. For, where every thing was a short time ago in doubt and confusion, all is now made clear by this wonderful instrument. Even in this day there are many honest cultivators of medical science, who do not believe in the value of the teachings of the microscope.

M. Velpeau died, having no faith whatever in its practical utility. A few years ago I was one of those benighted scoffers who believed it to be merely a scientific toy, with which to while away leisure hours. Fortunately, my ignorance was dispelled, and I now look upon the microscope as an essential to the daily duties of the physician.

With these prefatory remarks, I now beg leave to give you some illustrations of its use in treatment of the sterile state.

In the investigation of any case of sterility, there are three questions that must be settled at the outset, if we expect to treat it understandingly.

1. We must be sure that we have semen with spermatozoa.

2. We must ascertain if the spermatozoa enter the utero-cervical canal.

3. We must determine whether the secretions of this canal are favorable or not to the vitality of the spermatozoa.

For, if the semen does not contain spermatozoa, of course the uterine condition does not call for any treatment whatever. But if it does contain spermatozoa, and if they do not enter the cervical canal, then there is the question of operation or not, to permit their entrance.

On the other hand, if we should find spermatozoa in the cervical canal, then, as a rule, no operation will be needed; and if we should find them there, all or nearly all dead, then it is evident that the secretions of the utero-cervical canal poison them, and therefore the physical condition, giving rise to this abnormal secretion, must be searched out and treated.

The present advanced state of physiological knowledge warrants us in saying that conception is impossible without spermatozoa; and it is impossible if the spermatozoa can not pass into the cavity of the uterus; and, to these acknowledged truisms, I must add another, viz., that it is

equally impossible if they die in the cervical canal, or are dead when they reach the uterine cavity. It is, then, self-evident that these three points must be determined—it matters not what other complications may exist. Fortunately, as I have said before, they are all easily and quickly settled by the microscope. Without the microscope, it is impossible to determine either of them. Without the microscope, then, our treatment of the sterile state is simply blind empiricism. With it, our diagnosis becomes absolutely certain, and our treatment, at least rational. What, then, are the first steps in this investigation? How can we begin? Where shall we begin? Now, as it is upon this very point that I have been so stigmatized, I will tell you exactly how I manage this delicate affair.

Given a case of sterility for investigation, the physician examines the state of the uterus and its appendages. His patient may have a frightful dysmenorrhœa, a flexed cervix, a contracted cervical canal, something that would possibly prevent the passage of the semen to the cavity of the uterus. He may feel convinced, in his own mind, that conception can not take place unless some surgical operation be performed—perhaps incision of the cervix uteri. This operation is usually done to permit the passage of the semen into the uterine cavity. But in this, or in any case, what right have we to say that the semen does not or cannot pass into the cavity of the uterus? We must not take it for granted that it does not, simply because the os seems to be small; for we know that cases are recorded where conception occurred when the os barely admitted a small size probe; and we know very well that spermatozoa now and then pass along the Fallopian tubes, which ordinarily admit a bristle. If the semen enter the cervical canal, we may lay it down as a rule, that a dilation of the cervix by incision, or otherwise, is not necessary; but if it do not, it may be necessary. We may perform any rational operation for the relief of suffering, and for the restoration of health; but I insist that we have no right to perform any operation, or to institute any treatment whatever, solely with the view of cure of the sterile condition, till we have first settled the three propositions, above laid down, touching the presence and viability of the spermatozoa. To find out all at once, and, with the least delay and trouble, I usually say to the husband or wife, as it may be, "It is very important, before

instituting any treatment, to be sure that the seminal fluid enters the neck of the womb, for without this conception is impossible. We must also ascertain if the uterine secretions kill the semen; if so, a certain treatment will be necessary. If you will, then, send your wife here or come with her any day, five or six hours after coition, it will be easy to settle these points at once." In nineteen cases out of twenty, the wife presents herself the next day. The speculum is introduced (and when I say the speculum, I always mean the one that bears my name), and some vaginal mucus is removed by the syringe, and placed on an object-glass. Then some cervical mucus is drawn out and placed on another object-glass. These two specimens are then examined under the microscope. If we find spermatozoa, well and good; but if we find none, neither in the vaginal nor cervical mucus, our fears are at once aroused. What then is to be done? I simply say that I am not quite satisfied with the examination, and would like to see the wife again, at some future time, under the same circumstances. But suppose we find no spermatozoa on this second examination? Then two questions immediately arise: either, that there are no spermatozoa, or that the semen has all passed off before the case came under observation. Sometimes the semen is all instantly thrown off by the vagina, and then it would not do to pronounce the husband sterile until we are sure of a specimen of his semen for investigation. If I fail to satisfy myself on this point, I then explain the possibility of the semen all passing off in the act of rising and dressing, and show the absolute necessity of making the examination half an hour or so after coition, and before the erect posture is assumed. When the subject is presented in this plain, practical manner, and treated seriously, no man or woman of sense could oppose it; and with me, it has never, in a single instance, been objected to. When I am sent for to make examination, if I find in the vagina a fluid with the characteristic seminal odor, I am satisfied with the microcoscopic examination. I have never, in but two instances, been compelled to resort to Mr. Curling's plan, of asking the man to squeeze a drop of mucus from the urethra, on a bit of glass, immediately after sexual intercourse. But this is sometimes necessary; it is well to remember it.

If we eventually find that the semen contains no spermatozoa, then all uterine treatment is at an end. But if we are at last satisfied that it contains spermatozoa, then we must determine if these enter the cervix uteri, and if so, do they there find a fluid favorable to their existence alive? And all this can be done only by the microscope.

The question of the entrance of the semen into the cervical canal, and of the effect of its secretion upon the spermatozoa, can be fully and satifactorily ascertained only during a very brief period. We are sure to make a mistake if the microscopic examination be made just before the return of the menses; and why? Because, there is always a certain amount of fullness of the uterus—of engorement, so to speak—which precedes the menstrual flow; and the cervical canal may not admit the semen from mere turgescence of its walls. Besides, at this time, its secretions are almost sure to kill the spermatozoa even if they should happen to enter this canal.

Physiologists are generally agreed that conception takes place during the week following menstruation. Avrard says we have fourteen days of active uterine life and fourteen of uterine hypnotism. He says that conception can occur at any time after menstruation up to the fourteenth day, counting from its commencement. For instance, if menstruation should last for three days, then we would have eleven days for the possibility of conception. But if menstruation should last eight or nine or ten days, then we would have respectively but six or five or four days as the time possible for conception. After this time, the uterus, according to Avrard, lapses into the hypnotic state, when conception is impossible. While I am disposed to accept Avrard's dictum as the rule, I think I have seen exceptions to it, if we can always depend upon testimony seemingly reliable. Be this as it may, I am sure of this fact: if we wish to determine the effects of the cervical mucus upon the spermatozoa, we must make the experiment during the week that follows menstruation. About the fifth or sixth day after the flow is the best moment; for then the uterus is in the most favorable condition. The cervical mucus, which just before menstruation was perhaps thick and opaque, then becomes clear and translucent. If, by examination made at this particular period, we should find spermatozoa in the cervical mucus, we could safely say that it

will not be necessary to incise the neck of the uterus. But if the sperm do not enter the canal, then the probabilities are in favor of the necessity of some surgical interference. The semen may enter the cervix in great abundance, and we may find the spermatozoa all dead, even but a few minutes after coition. Then, as said before, we must find out the source of the poisonous secretion, and remedy it; for a vitiated secretion shows some organic condition requiring a special treatment.

When I wish to examine the action of the cervical mucus upon the spermatozoa, I order sexual intercourse in the morning—the dorsal decubitus to be retained for an hour afterward; and I expect a visit from my patient four or five or six hours after coition. Sometimes we find spermatozoa in great abundance in the cervical canal and not one living. (I have occasionally examined the mucus, six, eight and ten minutes after coition, and found all the spermatozoa dead.) Sometimes, we find half of them dead; again, only a third, again, two-thirds. Sometimes, in one portion of the mucus, every spermatozoön is dead; while, in another portion of the same sample with fewer epithelial scales, we may find them alive. Now and then, after treatment for a month or more, I have found the mucus drawn from the lower segment of the cervical canal full of living spermatozoa, and I have supposed that the case was cured; but when I came to examine that drawn from the upper segment of the canal, near the os internum, they were nearly all dead. This was evidently because the mucous membrane lining the lower segment of the cervix, being more easily reached and more thoroughly treated, had assumed a healthier character, and consequently its secretion was restored to a normal condition; while that higher up, and more difficult to reach, had not been so much improved, and hence its secretion was still abnormal—a condition requiring further treatment.

The vaginal mucus, by its natural acidity, kills very quickly every spermatozoön. I do not remember to have found one alive in the vagina, except when the examination was made very soon after coition; when, indeed, the vagina was full of semen but slightly mixed with other secretions. Examined three or four hours after intercourse, the spermatozoa found in that portion of the mucus of the vagina adhering to its walls are all dead. Indeed the normal

vaginal secretion seems to be a perfect poison for the superabundant spermatozoa.

When, after a month's treatment, I wish to know whether the case is cured or not—in other words, whether all possible barriers to conception are removed—I order sexual intercourse (just after menstruation) at night, and examine the cervical mucus twelve or fourteen hours afterwards. If the majority of the spermatozoa be alive and active, I have great hope of conception. Before dismissing a case as cured, I think it necessary to examine the mucus thirty-six hours after coition; and, if it is then all right, of course I suspend the treatment, and patiently wait the hoped-for result.

So much care is necessary for the removal of the mucus for the microscopic examination, that I may be pardoned for referring to it again. The patient is placed in the left lateral semi-prone position, as I have elsewhere so minutely described, and my speculum is introduced, and some of the vaginal mucus drawn up with a small glass syringe, previously washed out with warm water. This is deposited on the object.glass; the vagina is then cleared of all secretion, whether vaginal or cervical, the whole of the vagina and the os uteri being thoronghly wiped over with a pledget of cotton. This is for the purpose of guarding against the possibility of mixing vaginal with cervical mucus, which would, of course, spoil the whole experiment. The cervix is then brought forward either by the depressor or a tenaculum, if necessary, which enables us to look directly into the cavity of the cervical canal. The syringe is then again to be thoroughly rinsed in warm water; its nozzle is passed into the gaping canal for half an inch, and the cervical mucus in its lower segment is drawn out. The instrument is emptied, washed out again with warm water and reintroduced up to the os internum, and another portion of mucus is drawn out, provided there is any left after the first effort. Thus we have three specimens of mucus; i. e., one vaginal and two cervical. The cervical secretion should be clear and translucent, and about the consistence of the white of an egg. If it contain any little opaque specks of milky whiteness, it invariably poisons the spermatozoa to a greater or less extent. We sometimes find the cervical mucus perfectly clear, and yet poisonous to the spermatozoa. Here we would naturally expect to find excessive alkalinity of

the secretion; but I have not been able to detect it. In these cases it has seemed to me that the spermatozoa were killed—drowned, as it were—by the abundance of the secretion. I do not here allude to cases of uterine catarrh, where the secretion is very thick and albumino-purulent; for, of course, this is a deadly poison to the living principle of the semen. But I allude wholly to such cases as have been changed by treatment to a condition giving rise to a secretion seemingly normal, so far as ordinary ocular examination is concerned. Here the microscope is our unerring guide. The mucus may be clear, and perfectly normal in appearance; but, if it kill the spermatozoa, then there is still some point in the canal of the cervix, or in the cavity of the uterus, that gives out a vitiated secretion; and this must be found out and corrected before the case is wholly cured. When we find living, active spermatozoa high up in the cervical canal thirty-six or forty hours after coition, we can pronounce the case cured, so far as it may be by surgical means, and not till then.

It is time for us to pause, and consider if there is not something more to be done for the sterile condition, than to split up the cervix uteri. I look upon this operation as one of great importance, as one of the most valuable in uterine surgery, but I think we have followed too blindly the example and teachings of its illustrious author, Sir James Y. Simpson. For msself, I am now sure that I have cut open the cervix uteri, perhaps scores of times, when it was both useless and unnecessary; and I know that others have done the same thing. Do not misunderstand me. I speak here solely of the operation with reference to the sterile condition, when it would be wholly useless if the husband happened to be sterile, and certainly unjustifiable unless imperatively called for by considerations of health. Incision of the cervix for dysmenorrhœa is one thing; incision of the cervix for sterility, even if there be dysmenorrhœa, is another, aud it behooves us to draw the line of distinction in every case, and not to take it for granted that every woman is sterile who may have feeble health, and that every man is prolific who may be vigorous and enjoy good health. I am sorry to say that I have had the misfortune to incise the cervix in half a dozen cases of sterility, where I found afterward, to my great mortification, that the husbands are incapable of procreation, because their semen

had no spermatozoa, and that, too, since I have known the value of the microscope. In each case the operation was called for to restore health, but was totally useless for the relief of its incidental accompaniment, sterility, and would not probably have been submitted to for considerations of health alone, had it not been for the hope of offspring afterward. I made the mistake of operating on these cases, because the social position, moral character, and appearance of health in the husband, conjoined with the excessive dysmenorrhœa and utter prostration of the wife, led me to operate without the preliminary step of ascertaining whether there were spermatozoa or not. I wish others to profit by my mistakes; and I am less ashamed to tell you of them than I am to own them to myself. However, this can never happen to me again, and should not, after this warning, happen to any of my brethren. I know many men who have no spermatozoa, and cannot therefore become fathers. They are all strong, active men, in the prime of life, and all perform the sexual function with vigor. The very fact of their natural vigor and strong passions had been their ruin, for most of them had contracted urethritis during their early and unmarried life, and had suffered from its unlucky sequence, epididymitis.

Not very long ago (seven or eight years), I had the idea that sterility was essentially a female infirmity; that men were never sterile, except when impotent; and that any man, legally competent for the married state, was physically so for procreation. But the microscope unsettles and settles all such vague notions. It is natural to suppose that a strong, vigorous man is more fitted for procreation than a weak or puny-looking one. Some of the greatest lights of the profession have held such views as this. It was only two or three years before the death of the lamented Trosseau, that he said to me, in speaking of a case we had under consultation, "If our patient only had a man for a husband, all would be right." I subsequently found out that the husband's passions were strong; that his semen was perfect; that it entered the cervix in great abundance; and that the spermatozoa were there poisoned by a vitiated secretion. I mention this to show that we must not judge from appearances, when it is so easy to settle the question by the microscope.

Once I thought that the most common obstacle to conception was a contracted cervical canal, contracted at its outlet, at the os internum, or throughout its entire length. But, if I were now asked, "What is the most frequent obstacle to conception?" I should unhesitatingly say, "An abnormal utero-cervical secretion that poisons or kills the spermatozoa." I can call to mind numbers of cases where, in former years, I incised the cervix, when the operation was satisfactorily done, and yet the sterility persisted. In some of these I have now not the least doubt that the husbands were sterile, and in others I have as little doubt that the cervical mucus was poisonous to the spermatozoa. If I had then possessed the exact knowledge of to-day, how much more satisfactory would it have bave been for me—how much better for my poor patients!

Death from Chloroform.

Henry Gibbons, Jr., M.D., *Pacific Med. Journal*, reporting instances of fifty-seven deaths from chloroform, concludes as follows:

Since collecting and arranging the above cases, some others have come to my knowledge, which must, however, be deferred to another occasion. Here are 57 deaths, of which 32 occurred in the United States—5 of them in 1867, and 10 in 1868—19 in England, 2 in Paris, and one each in Dublin, Vienna, Prussia and Canada. They occurred in the following years: one each in 1850, 1856, 1858, 1859, 1860 and 1865; 3 each in 1862, 1864 and 1866; 13 in 1867; 24 in 1868; and in the remaining 4 the year is not stated. Omitting the two duplicated cases in Dr. Reeves' paper there are now 188 deaths from chloroform on record, of which 104 occurred in England and Scotland, 40 in the United States, 18 in France and the rest in various other countries. The mortality in each year is as follows: 1848, 8; 1849, 6; 1850, 8; 1851, 4; 1852, 7; 1853, 5; 1854, 10; 1855, 4; 1856, 4; 1857, 2; 1858, 7; 1859, 7; 1860, 7; 1861, 6; 1862, 15; 1863, 8; 1864, 11; 1865, 5; 1866, 6; 1867, 13; 1868, 24; and in 21 the year is not stated, but at least 15 of them are known to have taken place prior to

1862. The extraordinary mortality reported for 1868, which with one exception is confined to but two countries, the United States (10) and England (13), is worthy of the most thoughtful consideration. Great interest in this subject has, of late, been manifested. The medical journals seem to have been specially active in seeking out and publishing all cases of death from the agent under consideration, and to this fact is no doubt due, at least in great part, the large number above noted. The number of deaths in 1865 and 1866 is ridiculously small, if it is proper to apply such an adjective to so serious a matter.

Highly interesting and instructive facts may be learned by careful examination and consideration of the cases presented in this paper. They have reference to the time at which death took place; the mode of dying; the means used in the endeavor to prevent death; the amount of chloroform inhaled; the previous use of the anæsthetic, etc. They mainly corroborate the results of previous investigations, and will, therefore, be considered very briefly, as the space already occupied is far greater than was anticipated.

Death in a majority of the cases was sudden. In 28 it occurred almost immediately or within a very few minutes of the exhibition of the anæsthetic, and before or immediately after the commencement of the operation; in 4 it ensued just as the operation was completed; in 17 there is no definite statement upon this point, though in the majority of them the inference is that death was sudden; and in 8 the fatal result was more or less remote from the termination of the operation, or was not due entirely to chloroform.

Of these latter, one (XII) was that of a man who died seventeen hours after he had undergone an extensive operation—excision of the scapula—and death was probably by syncope; one, (XIV) a man who lying on his back, vomited, and the fluid not being thrown out of his mouth, some of it was supposed to have been drawn into the trachea, as violent struggling ensued; he became black in the face and died at once, evedently from asphyxia; in two cases (XXV, XXVI) the fatal result was in part, at least, attributable to the inhalation of impure chloroform, as the latter was found to contain hydrochloric acid, and the symptoms were those which might be caused by the action of irritant gases or vapors upon the air passages. Death

took place in twelve hours. In case XXXIX there was *stated* to be pulmonary phthisis; the patient "rallied for a time" and then expired; in case XL the patient after the operation spoke a few words, "gasped three times and was dead;" and in case XLI there was great loss of blood. Four of the deaths were in persons who took the chloroform while alone, and on their own responsibility, the length of time after the commencement of inhalation being, of course, unknown.

As regards the mode of dying, it was probably very generally by syncope, though of the 17 instances in which this fact can be ascertained, 9 were by syncope and 8 by asphyxia. The means to be used to prevent death when alarming symptoms have arisen, of course depend very much upon the mode of death. The same plan, however, seems to have been adopted in nearly all the cases, the records of which contain any mention of the subject. It consisted in drawing out the tongue; dashing cold water on the face; applying ammonia to the nostrils, though what benefit could arise from this, after cessation of breathing, is certainly not apparent; and artificial respiration, by Marshal Hall's method, by forcing air into the lungs either through the mouth or an opening in the trachea, or by other means. In a number of instances galvanism was resorted to, and in one or two the patient was bled. Over a year ago a physician in New York claimed to have saved the life of a patient who had apparently died while under the influence of chloroform, by placing his body at an angle of forty-five degrees. The description of this process was very obscure, but in all probabability it was the same as that recently proposed by Dr. E. L. Holmes, in an article in the *Chicago Medical Examiner, Sept., 1868*, entitled "Position in the Treatment of Chloroform Poisoning." In this article he advises when syncope has occurred, "to place the patient with the head downward on an inclined plane of at least 40°." The method of course would not be suitable in case of asphyxia, where there is evident congestion of the nervous centers, but only in those cases in which the brain is temporarily anemic, the gravitation of the blood overcoming this condition.

I recently administered chloroform (against my wish, but ether was absolutely refused) to a lady for a minor operation. She had been in the habit of using it for years to

allay neuralgia, and had taken it in large quantities. Pouring a few drops upon a handkerchief, I placed it near the nostrils and was obliged to repeat the process so frequently, before there was loss of sensation, that over two ounces of chloroform were used. Several times I almost resolved to discontinue the chloroform, but finally anæsthesia occurred and the operation was performed. On its completion, and after the inhalation had been discontinued, the patient became deathly pale and ceased to breathe. I immediately drew the head and shoulders from the pillow on the sofa upon which she was lying, and lowered them below the level of the body, when breathing was almost directly resumed, and the head was replaced on the pillow. But breathing in a few moments ceased again, and the same process being repeated it again returned, the head thereafter being kept low until the tendency to syncope had passed. This is a fair illustration of the advantage of position in the syncope of chloroform poisoning as suggested by Dr. Holmes.

In only seventeen cases is the amount of chloroform used designated. This was a drachm in three cases, from one to two drachms in eight cases, two and a half and four and a half drachms in two cases, eight drachms in three cases and ten drachms in one case. In another case it is simply stated that a large quantity was given. Eight of the patients, it is stated, had previously inhaled the anæsthetic, and in all but one of them it had acted kindly.

In conclusion it may be added, that in view of the facts noted in this communication, we cannot felicitate ourselves as Prof. Billroth recently did when he said in a clinical lecture, "Fortunately we may regard death from chloroform as a very rare occurrence, and one becoming more rare every year."

Contagion of Consumption.

M. Chauveau, Professor in the Lyons Veterinary School, continues perseveringly his researches on the contagiosity of tuberculosis. He has of late selected the intestinal surface as the field for his investigations, and through it, by introducing tuberculous matter into the circulatory current,

he has produced at will general tubercle. The *Union Médicale* reports that he lately purchased four handsome heifers, and he tuberculized three of them by causing them to swallow 30 grammes each of tuberculous matter taken from the body of an old phthisical cow. The rapidity of the result was extraordinary. At the end of twenty days the first heifer had lost flesh to a surprising extent, its pulse was quick and full, and it coughed incessantly. At the end of fifty-two days it was killed, and it presented perfectly marked tuberculous lesions, situated especially about the mesentery and intestine. The mesenteric glands presented infiltration in so high a degree, that many were of the size of the fist. Their total mass weighed 1,650 grammes. All the ganglia of the bronchi and mediastinum were enlarged, and the lung was full of crude tubercle. The other two heifers presented not less perfectly marked alterations, while the fourth, to whom none of the tuberculous matter had been administered, remained intact, and increased in flesh.

It is proved, therefore, that animals of the bovine species contract tuberculosis by digestive ingestion, just as they take carbuncle and cowpox, as sheep take the rot, as the horse takes glanders, and as man takes smallpox. The human digestive tube constitutes an easy channel for tuberculous contagion. If bovine phthisis be identical with tuberculosis in the human species, there is, in the use of the flesh of tuberculous animals, a danger to which the poor are more especially exposed.—*Medical Press and Circular, March 24.*

Carbolic Acid.

We think it necessary to put our readers on their guard against an incautious use of carbolic acid. It seems to be forgotten, sometimes, that this substance exercises a powerfully destructive action upon animal tissues, and that it is, in fact, a very strong caustic when concentrated. There is no doubt that many serious accidents have recently occurred from surgeons not being aware of the properties of the remedy they use so freely. It must also be remembered that the direct application of carbolic acid, even in a diluted form, to a granulating surface, will often delay cicatrization,

and tend to promote suppuration; whereas, if it is employed at a distance from the wound, it will tend to diminish the formation of pus. There is, moreover, a good deal of evidence to show that it tends to stimulate the circulation through the smaller vessels, and thus gives rise to hæmorrhagic oozing from recently cut surfaces, preventing their primary adhesion. If, however, it be properly applied, in a diluted form, to the wound itself, and in some permanent and non-volatile form to the external parts, it will be found to have a powerful influence in retarding and diminishing suppuration.—*Medical News.*

Two Interesting Parasitic Diseases.

J. H. Salisbury, M.D., Cleveland, Ohio, (*Boston Medical and Surgical Journal*), mentions two interesting parasitic diseases, with their treatment.

1st. *Chloasma produced by the Microsporon furfur* (Robin).—A cigar-maker, aged 30, came to him in January last, covered over the whole trunk, anteriorily and posteriorily, with brownish yellow maculæ, or spots of irregular outline, from the size of a pin-head to four and six lines in diameter. The spots were not elevated above what appeared to be the surrounding skin. The epidermis was soft and pultaceous, and appeared spongy when scraped with a scalpel. The colored cuticle peeled off like the epidermis from a boiled apple. The sheets and night-dresses were covered with furfuraceous scales. On microscopical examination, the epidermis was found filled with a fungus (*Microsporon furfur*), both in the spore and filamentous stages of development. The spores were developed in multitudes in the dark spots.

Treatment.—The patient was pale and emaciated, and was disturbed at night by alternate sweating and chilliness. A diet of rare beef and bread was ordered, and he was put on the following treatment:

℞. Acid, sulphuric aromat. ʒ iij.—S. Put two teaspoonfuls in half a pint of warm water, and wash the body and limbs all over every other night, and wipe dry afterwards.

℞. Nichols' sol. bisulphite of soda, ʒ iij.—S.

Put 1½ tablespoonfuls in half a pint of warm water, and wash the body and limbs all over every other night, and wipe dry. ℞ Tr. ferri chlorid. ʒ iij.—S. Take twenty drops in a full glass of water two hours after each meal. ℞ Tr. cinchonæ comp. ʒ vj.; potass. bromid. ʒ iv.—S. Take a teaspoonful before each meal.

On the fourth day of treatment the spots had entirely disappeared. Sickly spores and filaments remained in the epidermis, but further development seemed to be checked. Treatment was continued. At the end of two weeks, the vegetation disappeared, the skin was smooth and healthy, and the patient had gained several pounds in weight, and was feeling perfectly well.

2d. *Parasitic Disease of the Conjunctival Membrane and Epidermis of Cheek.*—A carpenter, aged 26, came to him with a diffuse inflammation, roughness and ædema of the eyelids and the surrounding soft parts. The inflammation and scaly condition extended over nearly the whole cheek, to the wing of the nose, and from this point, up the ridge of this organ, to the forehead, involving the entire eyebrow. At first it was considered a case of erysipelas, but on examination, the epidermis was filled with a fungoid growth, in the spore and filament stages of development. This vegetation extended to the conjunctival membrane lining the eyelids. The following was prescribed: ℞ Dilute citrine ointment, ʒ ij., glycerine, ʒiij.—M.S. Apply morning, noon and night, to the inside of eye and over the entire affected parts. ℞ Nichols' sol. bisulphite of soda, ʒ ss.; aquæ, ʒ viij.—M. With an atomizer, the spray of this mixture was thrown into the eye and over the cheek, for five minutes, morning and evening, after washing and before applying the ointment. ℞ Tr. ferri. chlorid. ʒiij.—S. Take twenty drops in a full tumbler of water two hours after breakfast and dinner. The use of sweets and all organic acids was forbidden, and a plain, substantial diet ordered. The patient rapidly recovered, and in three weeks there was not a trace of the parasitic growth.

Calabar Bean.

Dr. William Wright communicates to the Canada *Med. Journal* a valuable paper upon this agent, asserting it to be locally an antidote to belladonna, and generally to strychnia, and as a remedy in tetanus. Its efficacy in this latter

instance, however, is limited by the degree of lesion of the peripheral nerves, the agent having no control over the afferent or sensory nerves, and does not suspend their conductivity.

It would seem from the above that the calabar bean occupies a similar relation to tetanus as does chloroform, simply suspending the reflex irritability of the spinal cord, but as not moderating the irritation of the peripheral extremities of the nerves. As this is, in the vast majority of cases, the pathognomonic element in tetanus, we fear that the treatment of this formidable malady is "*adhuc subjudice.*"

OBITUARY.

DIED, in Philadelphia, on the 1st of April, after a protracted illness, ROBLEY DUNGLISON, M.D., Emeritus Professor of the Institutes of Medicine in Jefferson Medical College, and late Dean of that Faculty.

Few physicians in this country have established a wider reputation than Dr. Dunglison. His learning was extensive and varied; he was a ready writer and a voluminous contributor to medical literature. So popular, at one time, were his works, that they were adopted as text-books in nearly all our medical colleges. He was a highly courteous gentleman, and possessed uncommon tact and good sense, which contributed largely to the success of the institution, over the affairs of which he presided. He had many social qualities also which endeared him to a large circle of friends who will long deplore his loss.—*Med. News.*

DIED, in Albany, June 17th, Prof. ALDEN MARCH, aged about seventy-four.

The life of Dr. March is identified with the history of the profession, of which he was one of the brightest ornaments. He was alike distinguished as a scholar, teacher, and as a bold yet cautious and skillful surgeon. Beloved in private life, crowned with the highest honors of the profession, his decease will be widely and deeply lamented. To the Albany Medical College, of which he was the founder, his loss can with difficulty be replaced.

DIED.—In Miramac, Indiana, May 28, 1869, Dr. ASA M. PEARSON, aged thirty-seven years.

Dr. Pearson was a member of the class in Rush Medical College in 1856-7.